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AUTHOR	Jacobson, Willard J.
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ABSTRACT

Four types of science education research are defined: empirical, philosophical, policy, and developmental studies; problems and approaches related to each type are analyzed. Problem definition is discussed in terms of theoretical and practical problems. Arguments are presented for and against requiring doctoral students to define their own problems. Use of levels of significance in empirical research is discussed and a distinction drawn between statistical significance and decision making significance. The need for philosophical studies which analyze assumptions and the consequences of actions is stressed. Historical and comparative studies are advocated as aids to making policy decisions. A pattern for developmental research is outlined. The responsibility of the researcher to suggest the implications of all types of research is discussed. (EB)

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APPROACHES TO SCIENCE EDUCATION RESEARCH: ANALYSIS AND CRITICISM *

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Willard J Jacobson
Teachers College
Columbia University
New York, New York 10027

What is science education research? What are the approaches that we take to research in science education? How can our research be improved? Periodically, it is useful to reflect upon our experiences in our field, and since much of our research is still carried out by doctoral students, it is also important to reflect upon our experiences in guiding doctoral students in their research experiences.[1]

Science education research is the systematic attempt to define and investigate problems involved in learning and instruction in science. Four different types of science education research can be defined: (1) empirical, (2) philosophical, (3) policy and, (4) developmental or formative studies.[2] These types are interrelated and more than one approach to research is often used in a study. For example, developmental studies often involve empirical investigations, but the emphasis is on the development of instruments or instructional procedures and materials; similarly, empirical studies often involve the development of instruments or materials, but the emphasis is on the empirical investigation.

Each of these types of research will be defined, and some of the critical problems that we are encountering in each of these types of research will be briefly analyzed. First, however, it is important to consider "problem definition" for this is central to all research.

Problem definition. Problem definition is probably the most critical, difficult and frustrating dimension of research. Problems are defined out of problematic situations. We may sense that a situation is not as it might be, "feel" that there is something wrong, suspect an inconsistency, or wonder why something happens. But, to do effective research, the problem must be seen more clearly and defined more precisely.

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Eventually, the problem can often be stated in the form of a simple, clearly understood question. An inchoate and messy problematic situation seems much clearer after the distractions and irrelevancies have been peeled away. Once a problem has been clearly defined, the procedures are often a matter of logical deduction. Problem definition may be the most difficult and creative dimension of research. But, to state a problem clearly and succinctly is to be well along in inquiry.

There are two major sources of problems in science education that may be called the theoratical and the practical. In the theoratical, some lack, inconsistency, or vagueness is sensed as theory is applied in science education. For example, for several decades investigators have been studying conservation behavior among children using tasks drawn largely from mathematics and the physical sciences. We have recently recognized our lack of understanding of how children "conserve" using materials drawn from the biological sciences and have set out to develop and test such life science tasks. In the practical, some problem is sensed as we work with students, or struggle with program planning, or feel a lack of effective science materials. For example, there are many science educators who sense the practical need for developing more effective ways of stimulating the continuing intellectual growth of teachers. It is essential that we continue the struggle to define problems from both theoretical and practical problematic situations.

In both the sciences and education we are learning that it is often important to take a philosophical view in order to identify significant problems. In the sciences, the philosophical notions of symmetry and simplicity have helped stimulate the sense that something is not as it might be and to define the problem that can be tackled. In education and politics the "grass roots model" has been a useful approach. In this approach to problem definition, the possible effects of a proposed policy or practice upon an individual child, teacher or school system are deduced. Similarly, we can sometimes see problems more clearly if we shift reference frames and try to view problematic situations from different perspectives. The education of disadvantaged children, for example, is viewed differently by the struggling, harrassed mother who still generates hope for her children and the scholar who views the problematic situation in terms of the generalizations of his discipline. Group discussions are another way that the nature of the problems can be clarified as different individuals approach a problematic situation from different reference frames and help the individual to see his problem more clearly. Sometimes, a philosophical consideration of the ideal can help in problem delineation as someone has said, "Once we know what we want, we can find ways to get it". A philosophical view of a problematic situation can sometimes lift our efforts above mechanical puzzle solving to deal with that which is more significant.

To what extent should doctoral students who are preparing to become researchers become immersed in the difficult, time-consuming, and frustrating process of problem definition? This is a matter of great concern to those with responsibility for the development of doctoral programs, and there are at least two sides to the issue:

Pro: 1. If problem definition is the most difficult and important dimension of research, then the future researcher in science education should have experience in the struggles, frustrations and satisfactions of problem definition.

2. The philosophical analysis of a problematic situation in order to define the problems is an extremely important educational experience in which a student gains a more profound understanding of his field. Experience in problem definition may help students to become creative researchers rather than technicians.

3. It is important that a doctoral student be identified with his problem and believe in its importance. To spend two or more years working on a problem that is not your own and to live with it for the rest of your life can be a painful and limiting experience.

4. Although senior science educators have had more experience, the field will benefit from the fresh, brash questions that are often asked by the relatively unsophisticated.

Con: 1. Problem definition requires long and intensive experience in the field and, perhaps, can be done more effectively and efficiently by the doctoral adviser.

2. Many of our most significant problems are too big to be tackled by a single investigator and if we are to make progress in science education we must have teams of investigators tackling these larger problems. The field has benefited very little from many individual, unrelated research efforts and needs large scale team efforts under the leadership senior researchers.

3. The primary function of a doctoral program is to develop research techniques and skills, and this can best be done on a managable problem that may be a part of a team effort.

4. The interests and competencies of the sponsor of the doctoral study are factors to be considered in problem selection. Most sponsors are not competent to guide all kinds of educational research, and a student will gain more from working in areas in which his sponsor is able and interested.

It is almost a truism in education that it is insufficient to define problematic situations into "either-or" propositions. We will undoubtedly continue to have some doctoral students struggle with problem definition and hand other students problems that are parts of larger undertakings. And it maybe that, if we continue to recognize individual differences, we should use different approaches with students of differing aptitudes and proclivities. However, if we continue to be concerned with the education of creative researchers and leaders in science education, we should probably increase the number of students who view science education philosophically and struggle to define the problem out the inchoate problematic situation. Once having had this experience, they may be more likely to continue to do research throughout their careers.

Empirical research. Empirical studies usually involve the collection of data concerning the behavior of students, teachers or other subjects under study. Often, the data are used to accept or reject hypotheses. We are encountering a number of problems with empirical studies, and there are opportunities that are not being aggressively pursued.

Often, problems are defined in such a way that great efforts lead to seemingly meaningless or relatively inconsequential results. The classic example involves the large number of studies of the relative effectiveness of a laboratory approach as contrasted to a lecture-demonstration approach to science teaching. There have been over one hundred studies of this question, and the results have been inconclusive. Obviously, a good science teacher will use both laboratory and lecture-demonstration approaches to teaching, as well as discussions, readings, field trips, audio-visual materials, lectures, individual counseling and other approaches. In research, it would be more useful to study what approaches are most effective under what conditions to achieve specific objectives with various kinds of students. However, if the tremendous energies that have gone into the laboratory vs. lecture-demonstration studies have helped us to define our problems more clearly, they have not been in vain. On the other hand, if more attention had been given to philosophical analysis and problem definition, it is conceivable that we might have moved ahead faster and more efficiently.

A disturbingly large number of science education studies have reports of no significant differences. Most investigators expect to find significant differences, otherwise there would be little reason to undertake the studies, and the researcher dutifully reports his findings but has difficulty in masking his disappointment. Usually, few take cognizance of findings of "no significant differences" because the wrong problem may have been posed, or because we simply cannot afford to stop using new (or old) approaches to teaching and educational materials, or because we believe that the differences are significant enough for us.

In many cases the finding of "no significant differences" is rooted in the nature of man and his experiences. The individual is a product of his inmate potentialities and all of his past experiences. To design an experiment in which one variable among many influential factors is manipulated over a period of a year or less, to collect data using one of the few instruments available, and to expect significant differences to occur is unrealistic and possibly undesirable. The effect of the one variable manipulated over 75 hours or less in a classroom setting where there may be many more potent influences at work is miniscule when contrasted to the bank of experiences that even young children have had in the past. If students were to make the marked changes in behavior or attitudes that we hypothesize as a result of the limited educational imput in some of our studies might even be considered a sign of instability. However, when students begin study in areas where they have had limited previous experience such as in the study of a foreign language or chemistry we usually find significant changes in scores or measures of achievement closely related to these areas.

We should not abrogate our responsibility to make judgements in the choice of levels of significance. Too often, levels of significance are set mechanically at the .05 level without recognizing that levels that we wish to accept will vary and that in education there are risks involved in rejecting as well as accepting hypotheses. Clearly, the level of significance that we will accept in the testing of a method of teaching or new educational materials will be different from that used in testing drugs or airplane parts. However, we have often accepted uncritically a level of significance that may be appropriate in another field but conceivably unwarranted in our educational research. The level of significance indicates the probability that the observed result could have been produced by chance rather than by our treatment. In educational research we might often be justified to take the risk that the favorable results we observe in using a new science program or teaching procedure might have been due to chance rather than the program or procedure. In many cases in education, the consequences would not be catastrophic.

We should also recognize that there is risk in rejecting new approaches and materials. To reject new approaches or materials because changes as measured by admittedly imprecise instruments do

not meet mechanically set levels of significance can certainly demoralize creative innovators and possibly deprive students of educational experiences that could be of value.

In an applied field such as science education we might consider using both statistical significance and decision-making significance.[3] Certainly, we will not reject the use of "open-ended" approaches to laboratory instruction or field trips in earth science teaching because the difference in achievements of students when these approaches are used are not found to be significant, at the .05 level. Perhaps, we should set levels of significance for decision-making. It might be possible have knowledgeable practitioners help set the level at which they would accept or reject an hypothesis. Like statistical significance, the level of decision-making significance might be set before the investigation is underway. The development of levels of decision-making significance might lead practitioners to pay more attention to research and give our research efforts greater impact.

Research should be a creative undertaking and sometimes the unexpected findings are of the greatest significance. In a classic example, Oersted's discovery that a compass needle was affected by an electric current in a nearby conductor was of far greater significance than his more prosaic activities on that historic day. In science education, Lampkin's unexpected discovery of variability in the recognition of scientific inquiry was a much more important finding than the discovery of any patterns in the treatment of scientific inquiry in textbooks would have been.[4] There is a regrettable tendency in educational research to develop a tight research design, usually conservative and involving little risk, and then to patiently grind out the research, reporting findings that surprise no one. Certainly, there are many, often unpredictable, factors that affect almost any educational endeavor, and education is such a "soft" science that we can almost expect something unusual to happen as we carry out educational research. The creative researcher is alert to the unexpected, and our literature will be enriched as the unforeseen is reported.

If fields of research pass through stages of evolution, there would be general agreement that education is at an early stage of development. One of the early stages in most fields of science is a natural history stage in which a great deal of effort is expended on the description of phenomena. These descriptive studies make possible the development of classificatory systems and eventually postulations which can serve as a basis for hypothetico-deductive studies. Do we have enough descriptions of how children learn? Of how teachers teach? In many cases we may not even know what variables should be considered, and these might be uncovered through natural history studies.

To carry out natural history studies will necessitate the greater use

of anecdotal records, observational protocols, case study techniques and, as in anthroprology, call for long, book-length research reports. It will also mean that in our research reviews we should use scanning procedures that will recognize these kinds of reports as well as journal articles.

Philosophical studies. Philosophical studies may involve the analysis of assumptions underlying positions or actions, the nature of problematic situations and the delineation of problems within problematic situations, and of possible consequences of proposed actions. To a certain extent, philosophical studies may be seen as the application of intelligence to our problems. The most potent criticism to be made of this type of science education research is that there has not been enough of it.

Certainly, we need rigorous analysis of the assumptions that underly our present science programs. At times it would appear that program planners hold that the science subjects in the high school curriculum and their boundaries were handed down from on high rather than having been decided upon by fallible human beings in haphazard ways. For example, there must be some inconsistency in the fact that in a nation that has taken epoch-making steps in the exploration of the universe where science educators solemnly declare that the study of science should help young people develop a world view and few students will have an opportunity to make a systematic study of astronomy. This is one of many issues related to science programs that deserves rigorous examination.

One of the functions of philosophical analysis is to clarify possible consequences of actions. Certainly, it is a characteristic of intelligence to try to predict possible consequences. In fact, some pschologists might say that this is a characteristic of operations at the formal level of intellectual development. Some philosophers have suggested that the analysis of possible consequences in social situations leads to postulations from which we can deduct hypothetical consequences and the tests to determine the extent to which these hypothetical consequences actually do occur. In other words, the analysis of possible consequences lays the foundations for social sciences, and most research in science education can be considered social science research. While the mechanistic, reductionist philosophies of science have been of great utility in the physical sciences, in dealing with social situations where complicated human beings both as individuals and as groups occupy the center stage and where the whole is almost always greater than the sum of the parts, the philosophy of pragmaticism with its emphasis upon the analysis of possible consequences provides a promising base.

A philosophical analysis of possible consequences might serve to dampen the extreme swings of the pendulum in our field. For example, the severe criticism of science teachers and educators as lacking in

rigor and possessing low standards led some of our more pliable colleagues to introduce kinds of rigor and standards that even the most savage critic could not have wanted. Part of the retreat from society, rebellion against our culture and flight from science on the part of some of our most intelligent and sensitive young people can be attributed, at least in part, to this highly unfortunate extreme swing of the pendulum. At the present time, we may be seeing young people who are almost crying out for a form of discipline--but a discipline rooted in our experience in analyzing situations and suggesting possible consequences. For example, in dealing with the environment there is the clear necessity to analyze the possible consequences of our actions, and the sudden surge of concern for the environment on the part of young people calls for leadership in the analysis of situations for the possible consequences of proposed actions. The philosophically inclined science educator has an opportunity of abstracting from such experiences a discipline for analysis and action.

There is a need to place our research and its results in perspective, to relate it to other kinds of experiences, and to extend the results to logical implications for action. For example, many researchers apparently do not believe their research because their actions are not consistent with their findings. But,there is always a danger that people outside the field of education will take the results much more seriously. Some educators have "discovered" that education isn't as potent a force for individual or societal development as some enthusiasts have maintained. These "findings" have been used by critics to *downgrade* the importance of education. However, some of these educators will move heaven and earth to make certain that their children get a good education. What interpretation should be placed on these findings? On the other hand, the implications of findings are sometimes not extended into action. For example, an analysis of developments in agricultural technology indicated that there could be a mass exodus from rural areas to the cities. A logical extension of these findings to action could have prevented some of the tragedies that are now unfolding. Philosophical analysis could add meaning to our research and make more likely the responsible utilization of its results.

Policy studies. Policy studies in science education include historical studies in which policy decisions and their consequences are analyzed, comparative science education studies in which we try to learn from the experiences of others, and futuristic studies in which we try to project possibilities and predict consequences of various proposed policies for the future. The years since 1955 have seen more activity in science education than any other comparable period; it would be unfortunate if we were not to make every effort to profit from this experience. Hopefully, we will have new policies and grander efforts in the future; certainly, we should use as much intelligence as we can muster to insure that we will do better in the future than we have in the past.

Historical studies should be viewed primarily as systematic attempts to learn from the experiences of the past. Too often, historical studies have served a conservative function to show all the things that have been tried in the past with limited success forgetting that usually no new venture is an exact duplicate of a past experience. A much more profitable approach to historical studies is to seek for ideas and to try to learn from what has happened in the past. Undoubtedly, we would have been more efficient and effective in the past decade if more systematic attempts had been made to profit from the experiences of insightful experimenters and clever innovators of the last few decades.

Usually, the most valuable historical studies are critical and analytical as well as objective. Sometimes, historical studies are written by those who played a central role in an educational development and read like an apologia for what was done. While these "memoirs" can be valuable source materials for the historian, they are no substitute for analytical history.

Through comparative science education we can learn from the experiences of others. To give one example, India is embarked upon an unparalleled educational effort to check population growth. Other nations should do the same, and we have much to learn from the planning and programs, successes and failures of our Indian colleagues. More science educators should be encouraged to undertake comparative science education studies when they visit and study in other nations, and we should develop better mechanisms for reporting this research. There is a rich lode of experience in science education in other nations, and we can learn a great deal from an analysis of these efforts.

More attention needs to be given to futuristic studies in science education. In other fields, imaginative scenarios are developed that explore the possibilities and consequences of various proposed policies. The development of such scenarios sometimes make explicit possibilities that would otherwise not be foreseen. It also makes possible more intelligent choice between alternatives. There have been very few such futuristic studies in science education. Greater effort in this direction might help us to develop more effective policies in the future.

Developmental research. Developmental studies involve the preparation of new educational materials, procedures, or programs and systematic tryouts in which feedback is gathered that can be used for improvement. They have the attraction that they can lead to a perceptible improvement in the education of students. At a time when there is a call for "relevant" research and education this is an important consideration. Although developmental research has not had as prominent a role in the course content improvement projects as we might wish, some valuable insights have been gained.

A general pattern for developmental studies has evolved. The pattern that follows is often altered to meet special conditions, but the major elements are usually included:

1. Exploration for ideas. The innovators usually have a basic idea that they want to develop and test. However, it is useful to search the literature for additional ideas that may make the development more useful and to interview others who have a record of innovative work or practical experience in the field.

2. Preliminary planning. The innovators outline the content of the innovation, assign responsibilities for development, and plan procedures for development and testing.

3. Development of preliminary form of innovation. The developers write or build the innovation. Drafts or preliminary models are often discussed and criticized in in-house seminars. Where appropriate, laboratory tests are run to make certain that the innovation "works". The content validity is checked.

4. Small scale tryouts. The preliminary form of the innovation is tried by a small group of teachers under field conditions in which the innovators are deeply involved. It is helpful if the teachers and others involved in these tryouts have some understanding of the total enterprise and no reluctance to express their suggestions and criticisms.

5. Preparation of the trial form of the innovation. Engineering and production problems involved in large scale manufacturing may be considered in the preparation of the trial form.

6. Field trial of innovation. The innovation is tried with populations that are believed to be roughly representative of the total population that may use the innovation. The general appropriateness and applicability of the innovation are reviewed. Some new ideas may be generated out the experience in a variety of localities. In some cases, summative research is carried out to establish achievement norms for different populations. Problems of distribution and of use under general field conditions are detected.

7. Preparation of version for dissemination. Revisions are made in light of field trials, and a version for general dissemination is prepared. Often, this version is a commercial edition.

8. Analysis of experience. Those engaged in developmental studies have an obligation to analyze their experiences so that others may learn from them.

In terms of process, our experience seems to indicate that the interchange in the small scale tryouts between innovators and with teachers or students who have some understanding of what is being attempted is especially useful. It is very difficult for someone who has not had an opportunity to become aware of the total nature of the innovation and the purposes it is to serve to make many helpful suggestions or criticisms on a feedback form. Articulate cooperating field testers who have an understanding of the innovation can make many helpful criticisms and suggestions in group discussions.

There is no mention of evaluational research in which the effects of using an innovation and other materials or procedures are compared. Sooner or later, teachers and instructional leaders have to make decisions as to the choice of materials and procedures, and it would be desirable to have these decisions based upon research. It is difficult, and usually suspect, for the developers to undertake this kind of evaluational research. An argument can be made for having independent groups undertake this kind of evaluation. On the other hand, decisions about the use of innovations almost always have to be made in light of local conditions. In many cases, it may be more desirable to have school systems and other consumers evaluate innovations by undertaking systematic pilot tests of innovations under local conditions.

While we no doubt should continue our efforts to develop better science programs, there are important reasons for suggesting that additional effort be devoted to the development of units that can be used to build programs. The high mobility of the American population which results in some schools having an annual turnover of students exceeding 100%, the high absentee rate in many of our urban schools, the difficulty that students encounter if they have missed some key element of a tightly and logically organized program, and the desire of local communities to develop science programs that they believe best meet their needs are arguments for the development of smaller units of instruction.

We need developmental studies that will help us in our research. A wider range of effective research instruments and tools is needed. Science education researchers have been forced to use instruments that are partially inappropriate because of the limited choice available. The development of effective instruments can provide the keys that can unlock whole areas of science education research.

We have learned again during this period of science curriculum development that the teacher is of central importance in a learning situation, probably more important than the materials, facilities, buildings or type of school organization. It may be that the teacher and how he teaches is more important in science instruction than in many other areas of the curriculum because so much of what

we want to convey to students takes the form of general approaches to the universe, processes in dealing with problems, and attitudes toward fact and fancy. It may be that the most effective way to convey these elements of science instruction is through a teacher who can provide a model for his students. We need developmental studies in teacher education which will help future teachers to learn how to provide such models.

Implications and suggestions for further research. The educational researcher has the responsibility to suggest the implications of his research. We have an admirable ethic in research that the researcher should not go beyond his data in reporting his results. However, this can be carried to a cautions, conservative extreme. The researcher also has the responsibility to utilize his data to the fullest. It is irresponsible for a science education researcher to undertake an important study and then retreat from suggesting the implications of his findings. Instead, the researcher owes it to other researchers and practitioners in the field to suggest what he thinks his findings imply.

The results of research should be clearly stated, the bases for the statement of implications carefully formulated, and the implications of research unambiguously labelled. Other researchers and practitioners should be able to go to the research report, locate and understand the report of findings, check the author's statement of implications against the findings, and possibly make their own interpretations.

One of the most important results of research are the new questions that are raised and the new opportunities for research that are made evident. Science has been likened to an expanding sphere. As the sphere grows, the area for active and productive research increases. In science education research, the statement of "Suggestions for Further Research" should be taken seriously and regarded as one of the most important results of the research.

The researcher can also suggest to practitioners ways that they can check his results in their situation. There is always danger in extrapolating to the universe. But, if we suggest ways our results can be checked, this danger is minimized. Also this is a way of linking action research on the local level to more basic research. It may also be a way of encouraging a research approach to decision-making in science education.